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Innovation, Integration and Impact

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The place of the personal in qualitative research

Amanda Coffey

This issue of *Qualitative Researcher* brings together articles that all attend, in various ways, to the personal dimensions of qualitative research practice. Of course it has long been recognised that qualitative research has (auto)biographical dimensions. Qualitative researchers are, more often than not, concerned with observing, narrating and writing lives and experiences; and as such act as biographers of sorts. At the same time qualitative research, in its purest form, is experienced by and embodied through the researchers themselves.

Personal narratives of fieldwork journeys are certainly not new, although they have conventionally been positioned as a parallel trope (to the more formal analytic text or research report). Field notes and journals have always been used to record the emotionality and personal identity work associated with prolonged research engagement. Qualitative researchers have also reflected upon and written about the self in or as texts of the field through (for example) confessional tales and methodological accounts, as well as in the more recent turn towards autoethnographic practice. Some of these genres pay particular attention to the researcher as author;

some acknowledge the presence of the researcher in the research process; others centre the researcher's life and experiences as subject of the research as well as producer of the text.

What these all have in common is an understanding that the researcher self is part of the qualitative researcher endeavour, and that the experiences of the researcher are integral to data collection and analytical insight. Where contestation occurs it is in the extent to which our private experiences are (made) public – in both the framing of our research problems and our writings. The contributors of this issue thus contribute to debates about the possibilities, challenges and problematics of recognising the intertextualities between qualitative research and autobiographical practices.

Methodological innovation is about the development and evaluation of new methods for collecting, analysing and representing social scientific data. It should be recognised, however, that a commitment to innovation also necessitates a transparent approach to social research; and a commitment to engage in open, and at times uncomfortable, interdisciplinary dialogue.

Call for Papers
Digital Research Methods in the Social Sciences
21 - 25 May 2007, Cardiff University
EUROQUAL



EUROQUAL is holding the first in a series of workshops on Qualitative Research in the Social Sciences. The first workshop will be on Digital Research Methods. Please send an abstract (200 words) together with a 300 word biography to Helen Greenslade (Project Coordinator) at Euroqual@cardiff.ac.uk by Friday 23rd February 2007

Topics include:

- qualitative research in cyberspace
- researching online communities
- hypertext and hypermedia applications
- computer-aided qualitative data analysis
- studying computer-mediated communication.



Arguments against Auto-Ethnography

Sara Delamont

Introduction

This is a deliberately controversial paper: I develop a series of arguments against autoethnography. As Leon Anderson (2006) has written in a recent *Journal of Contemporary Ethnography* the last 15/20 years have seen a growth of autoethnographies. By that I mean texts which claim to be *research* but the topic/focus of the research is the author herself or himself. This trend is particularly associated with Carolyn Ellis and Art Bochner (1996). I mean "studies" like Lisa Tillmann-Healy's (1996) reflections on *her* bulimia, or the piece by Carolyn Ellis (2002) on *her* response to Sept 11th 2001. Since *Composing Ethnography* (Ellis & Bochner, 1996) there has been an explosion in auto-ethnography. Journals such as *Qualitative Inquiry* and *Qualitative Studies in Education* regularly feature autoethnographic papers. In the first edition of Denzin and Lincoln (1994) there was one index entry for Autoethnography, in the second (2000) edition there were thirteen with a chapter by Ellis and Bochner (2000), while in the third (2005) edition the number had grown to thirty seven, and there was a dedicated chapter by Jones (2005).

I see this as almost entirely pernicious. Autoethnography is essentially lazy – literally lazy and also intellectually lazy. I have six objections to auto-ethnography four listed below and two others addressed after the empirical material.

1. Auto-Ethnography cannot fight familiarity – it is hard to fight familiarity in our own society anyway even when we have data (see Delamont, 2002).
2. Auto-Ethnography is almost impossible to write and publish *ethically*: when Patricia Clough published poems about a lover's genitalia, did he agree to them, when Carol Rambo Ronai (1996) published 'My mother is mentally retarded' did her mother give 'informed consent'? Other actors cannot be disguised or protected. Readers will always wish to read autoethnography as an authentic, and therefore 'true' account of the writer's life, and therefore the other actors will be, whatever disclaimers, or statements about fictions are included, be identifiable and identified.
3. As Paul Atkinson (2006) argues research is supposed to be analytic not merely experiential. Autoethnography is all experience, and is noticeably lacking in *analytic* outcome.

4. Autoethnography focuses on people on the wrong side of Becker's (1967) classic question ('whose side are we on?') Autoethnography focuses on the powerful and not the powerless to whom we should be directing our sociological gaze. In these four ways autoethnography is antithetical to the progress of social science, because it violates the two basic tasks of the social sciences, which are: to study the social world - introspection is not an appropriate substitute for data collection; to move their discipline forward (and, some would argue change society).

I will illustrate my position drawing on my current fieldwork to contrast interesting social science questions and introspective dead ends.

I am currently doing an ethnography of how the Brazilian dance and martial art capoeira is taught and learnt outside Brazil – what Assuncao (2005) calls diasporic capoeira – with Neil Stephens (Stephens and Delamont, 2006).

Imagine four locations: a small kick boxing gym in a working class neighbourhood of Tolnbridge, a community centre in a former school in an African-Caribbean neighbourhood in Cloisterham, a large sports centre in a working class neighbourhood in a very snowy Utrecht, and Rotorua, the tourist resort with sulphuric hot springs in the centre of the North Island of New Zealand. The kickboxing gym is known as 'Gladstone's' after the African-Caribbean joint owner and senior kickboxing teacher, who is a man in his forties. His partner Haralambos, a Greek Cypriot owns lots of rental properties. The community centre, called after Bob Marley, has an airport style metal detecting arch that can be placed in the entrance, and carries a huge sign saying 'No drink, no drugs, no knives, no guns "No Search, No Entry". No entry after 2 am'. The sports centre in Utrecht is hosting the first all-women's capoeira festival ever held in Europe. Two hundred women have travelled to The Netherlands, through an unexpected snow storm, to be taught by some of the best women capoeira teachers in the world, who have themselves travelled from Brazil, the USA, and all over Europe. In Rotorua, the biggest celebration of 'Latin' dance and music held in New

Zealand is taking place over Easter. Hundreds of New Zealanders have gathered to take master classes in dance (tango, salsa, samba, bossa nova etc.) and music (especially drumming), stage a huge carnival parade, watch demonstration of Latin dance, and enjoy themselves.

What do these four settings have in common? Capoeira: being taught at Gladstone's, in the Bob Marley Centre, in Utrecht and in Rotorua; and me, doing fieldwork. Who and what is sociologically interesting in these four places? Brief extracts from the fieldnotes, or the reflexive fieldwork diary, sketch out some social science issues.

At Gladstone's: Gladstone and Haralambos are certainly interesting: a life history of either could be a sociological classic. On Wednesdays the capoeira follows a women-only kickboxing class: the teacher (Shannon) is a national kickboxing champion herself, the women taking the class range in age from 14-40, are a mix of students and locals, and include at least three different ethnic groups. The capoeira students waiting in the street outside for their lesson are also a mix of students and locals, are 18-35, and vary greatly in their knowledge of capoeira. Their teacher, Achilles, is Brazilian living thousands of miles from his home and family, having abandoned the career he qualified for in Brazil. The passers-by, who glance at the capoeiristas and the strange musical instruments they carry, are residents of an inner city, multi-racial area of a rapidly changing city: they include a university professor and his wife out for a Chinese meal, three British Asian teenage boys who come every lesson to mock but will not join in, and a man who often brings his son, aged 6, to look at the class for a few minutes while they are en route to the chip shop. An autoethnographic question that arises is: I very much want the three teenagers to join the class, but I am not sure why I feel that desire.

In Cloisterham, the Bob Marley Centre is located in a neighbourhood that is notorious for race riots, drugs and crime. At 5.30 on an August evening, however, the streets are almost empty and the grounds of the centre are deserted. There are two buildings, both locked until a middle aged African-Caribbean

with a strong Cloisterham accent, appears to open the sport and dance hall for the capoeira class at 7.00. He speaks to me, and I explain why I am so early: 'I'm going to interview the capoeira teacher before the class for a project'. The caretaker starts to stack the chairs, sweep the floor and generally prepare the room for an exercise class. At 5.45 Achilles arrives to be interviewed, at 6.45 the twelve capoeira students begin to arrive. When the class is assembled there are five nationalities other than British, students or graduates of five different disciplines, and an age range from 18 to 40 (excluding me). When I leave to walk back to Cloisterham station, I notice that the shops, bars and cafes all have versions of the 'No drugs, no drink, no knives, no guns' notice posted above their entrances. An autoethnographic question that arises is: why don't I feel scared in this dangerous neighbourhood?

In Utrecht there are about 200 women capoeira learners, about 20 women teachers, plus five or six male teachers and about 50 male capoeira learners – mostly, as far as I can see, either from the local club that is hosting the event, or friends of the women visitors. The rules of the all-women meeting are carefully specified in writing, and the general air of excitement is much more apparent than any depression caused by the sudden snow fall which has disrupted even the Dutch public transport system. An autoethnographic question is: how do I feel about being at an all-female event?

In New Zealand, there are locals (with Maori and Pakeha) and tourists, as well as those who have come for the Latin festival. These visitors include several Pakeha samba bands, as well as all the dance enthusiasts. New Zealand's own capoeira group, which has branches spread out from Auckland to Dunedin, is having a reunion and celebration, with two visiting teachers from Brazil. They are offering some classes at the main festival, but are based in a Maori cultural space, a *marae*, about twenty minutes walk from the town centre, where they are camping, and training together. The warden of the centre, a Maori man of enormous size, watches the classes, as do several children. The capoeira master is a New Zealander who saw capoeira in California, went to Brazil to learn to teach it, brought it back to New Zealand and blended it with Maori and Pasifika elements into a New Zealand fusion. At the Latin festival, an African-Brazilian from Salvador de Bahia, Bira Reis, a pioneer of samba reggae, and member of

Olodum, teaches master classes in percussion: samba, samba-reggae, maracatu, condoble (the African-Brazilian religion) and capoeira. An autoethnographic question that arises is: how do I feel about being so musically inept that I cannot beat a maracatu rhythm even for a master teacher?

There is no need for me to labour the point that each of these four settings offers literally dozens of interesting sociological research projects. Anything about me is clearly of no sociological interest at all compared to all the many research questions that arise from those four settings. I offer below two questions from each setting as examples. In each case one question needs no knowledge of the setting, and so leaps out at anyone with a gramme of sociological imagination. The second question derives from my deeper knowledge of the setting, but its sociological importance will be clear once it is posed.

1. Two questions to follow up in Tolnbridge:

a) What prevents the three young British-Asian men from joining the class? It clearly fascinates them but they cannot or will not join it, despite regular invitations from Achilles. Underlying that are big questions about age, ethnicity, social class and multiculturalism in Tolnbridge.

b) I know from my conversations with the young men in Achilles' class that Gladstone and Haralambos are unpopular with some (all?) other martial arts teachers in Tolnbridge, and some of those entrepreneurs who sell martial arts kit. Haralambos is widely known as a landlord too. What underlies such tensions and what consequences are there? Underlying that are some of the big issues about race, ethnicity and money in the city, and questions about the 'hidden economy' of martial arts teaching and kit supply.

2. Two questions to follow up in Cloisterham.

a) Is the Bob Marley Centre really in a dangerous neighbourhood? How far are the metal detectors and signs 'necessary'? If there are 'dangers', what form(s) do they take, when are they acute and when dormant or latent? Underlying that are classic sociological questions about the city, race, crime, violence and drugs.

b) As I walk through the neighbourhood it looks as though gentrification is beginning: that raises questions about whether the capoeira students, who are mostly graduates of the elite university, see the

neighbourhood as a place they could ever live in? The race riots happened before they were born, and may not be part of 'their' mental map of the city at all, although they are at the back of mine. Underlying that is the perennial topic of social change in the city.

3. Two questions to follow up from the Utrecht festival.

a) What are the attractions and repulsions of an all-women festival for women capoeiristas? This opens up big questions about learning, teaching and embodiment in mixed and single sex groups.

b) The small number of men at the Utrecht festival were segregated into separate classes and when there were rodas, (the circle within which two people play an opponent competitively) there were special rules to prevent men dominating that space: only one man to play at any time, women experts to sing the solos and play the lead instrument. As men normally outnumber women by three to one in classes, and dominate the rodas, how was Utrecht experienced by the men?

4) Questions arising from the Rotorua experience:

a) Bira Reis is an African-Brazilian from a poor neighbourhood who has become part of the globalisation of 'world' music. What are the consequences of globalisation for indigenous musicians?

b) How do Maori and Pasifika peoples feel about Pakeha attempts to syncretise their cultures with Pakeha and other 'alien' cultural forms such as Brazilian music, dance and capoeira?

Conclusions

I said I had six objections to autoethnography:

1. It cannot fight familiarity
2. It cannot be published ethically
3. It is experiential not analytic
4. It focuses on the wrong side of the power divide

I now add two other objections:

5. It abrogates our duty to go out and collect data: we are not paid generous salaries to sit in our offices obsessing about ourselves. Sociology is an empirical discipline and we are supposed to study *the social*.

6. Finally and most importantly 'we' are not interesting enough to write about in journals, to teach about, to expect attention from others. We are not interesting enough to be the subject matter of sociology. The important questions are *not* about the personal anguish (and most autoethnography is about anguish. Soci-

ologists are a privileged group. Qualitative sociologists are particularly lucky as our work lasts: what sociology is remembered for – the great ethnographies: *City of Women* (Landes, 1947), *The Silent Dialogue* (Olesen and Whittaker, 1968), *Street Corner Society* (Whyte, 1955), *Boys in White* (Becker *et al.*, 1961), *Tally's Corner* (Liebow, 1967).

Autoethnography is an abuse of that privilege – our duty is to go out and research the classic texts of 2050 or 2090 – not sit in our homes focusing on ourselves.

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Autoethnography and untold stories

Ruth Bridgens

Introduction

At the recent Cardiff ESA conference 'Advances in Qualitative Research Practice', Sara Delamont gave a plenary entitled "Through the Lebanon gate: arguments against autoethnography", in which she described autoethnography as blinded by over-familiarity, lacking serious questions and analysis, and dull. This attitude discourages researchers from recognising their motivations, thoughts, and feelings as integral to their research, and may occur because 'subjectivity' is felt to be threatening and outside the sphere of 'objective' research. Mary Douglas has argued that "all margins, the edges of all boundaries which are used in ordering the social experience, are treated as dangerous and polluting" (Douglas 1999 [1975]:113). My PhD research on the experiences of people

who had polio as children in the 1940s and 1950s, and now have new deterioration called postpolio syndrome, encapsulates a similar reaction by society to the ambiguous and threatening. Children, who recovered well, although with some disability, were discouraged from thinking, feeling or speaking about polio in order to get on with 'normal' lives. In this paper, I will briefly explore what might lie behind these similar reactions and how research can begin to penetrate further into cultural stories of 'objective research' and the 'triumph over adversity'.

I also gave a presentation on autoethnography at the ESA conference in which I argued that sometimes it is only through autoethnography, autobiography or narrative studies that some experiences,

which are ignored, distorted or silenced because of the discomfort they cause, can become known and understood. Many marginalised groups have their own coherent culture, but others remain isolated, on the border's edge, and have their stories denied. Survivors of wars, serious illnesses and accidents, especially children, often find themselves in this situation. They are told to forget the past as they escaped or were cured. So who finally recognises and tells these stories?

In my presentation, I compared adult narratives and autoethnographies of childhood polio, childhood emigration, and being hidden as children during the Holocaust, groups who tell similar stories of being lucky to have survived. For many years, no one wanted to hear about their experiences and they often

felt they had no story. When I asked one woman I interviewed if her husband and grown children knew the polio story she was telling me, of several years in hospital, she said "I don't think anybody is really particularly interested to know".

I was aware of the similarities of these often minimal stories because of my own background which combined childhood illness and emigration -- a silence surrounding my Russian Jewish grandparents' emigration to the US in 1906 and my recovery from mild polio as a two-year old which was never talked about. During my research on polio narratives, as I heard stories of children not being told about their illness and not being listened to, I thought of new connections to explain the family silence which had stopped me asking questions, or thinking there were questions to ask. Using their stories to answer these questions also helped me understand the contradictions and confusion the interviewees were facing recognising polio as part of their lives and their new deterioration.

Finding a story

For example, one interviewee, Jennie, said that she had not been affected much by polio, but remembered a feeling of resentment when she was in hospital at age five that her parents never did quite the right thing, and afterwards never listened to her story. Later in the interview, she described in great detail the mother of a disabled boy she had taught, concluding:

Jennie: she didn't see what was important. But she just had to show *him* that she did all she could ... to help him with his problem. Except that she wasn't doing it the right way.

After the interview, Jennie sent me an e-mail, having made the connection between her story and the disabled boy. She wrote: "I suddenly see the links between all this and the boy with cerebral palsy that I talked about...His mother spent all her time putting her case, her point of view, without listening to, or doing what was best for him. As I saw it my mother did the same thing...In my role as a support teacher for children with special needs...I saw myself as 'being there for them'. I would have liked someone to have been 'there for me'".

In order to silence children and avoid emotions like sadness, these children are told to ignore the past, think positively

and see themselves as more fortunate than others. Gordon and Paci (1997) argue that the silence surrounding cancer disclosure in Italy involves a difficult balance of gains and losses for patients and families in order to minimise suffering and retain some sense of normality. This analysis can be used to understand the complex protection from feelings exercised by parents, children, medical professionals and society about childhood illness.

After studying several interviews in which a child's feelings were ignored, I connected the only two fragments of information I knew about my mother's family in Russia -- one, related by my mother, that she had two brothers whose names she never knew, who had mysteriously died as small children in Russia; the second, told to me by a cousin after our parents had died, that her father, my mother's eldest brother, who was seven when they left Russia, had had nightmares all his life of Cossacks riding by on horses with Jewish babies on their swords. Whether the brothers had been killed or not, had the uncomfortable silence about the past caused shame, like the shame many children felt about polio, and led to my mother being silent about my illness? Ruth Behar, in *The vulnerable observer* (1996), similarly combines childhood emigration and disability to explore autoethnography and untold stories.

The girl in a cast

In the chapter 'The girl in a cast', Behar (1996) describes two major disruptions in her childhood -- the emigration of her Cuban Jewish family to New York when she was five, and a serious car accident when she was nine, which kept her in her bedroom in a cast for a year. When the cast was finally taken off, she felt she was pressured into walking too quickly without her fears being acknowledged, possibly because her parents, like my parents, were focused on their children belonging in a way they did not. In her thirties, Behar began having panic attacks and realised that her experience in the cast had affected her more deeply than she had imagined. She remembered that at the time of the accident:

'I was not allowed to feel sorry for myself because it might have been worse...The adults kept telling me I should be happy... It's just a broken leg... Imagine if the leg had needed to be cut off. Or, worse, what if I ended up a vegetable? I had to be grateful' (Behar, 1996: 106).

Behar (1996: 134) uses an image from Salman Rushdie, of the exile's homeland being imaginary, to think about childhood and how "we are encouraged to put the child behind us, to disbelieve our own stories and our own childhoods". However, reading Oliver Sacks' *A leg to stand on* (1984), she realises she wants "to retrieve the child I once was and give her the understanding, the words, the knowledge, I now had" (Behar, 1996: 129). Behar argues that the border between adult and child, like the border between cultures, and between researcher and researched, needs to become more fluid and the knowledge of childhood remembered.

Hidden Holocaust children

Claudine Vegh, in *I didn't say goodbye* (1979), wrote one of the first accounts of the lives of children who had been hidden from the Nazis, in order to make a similar connection to childhood and a lost past. These stories had not been told because the children were not considered Holocaust survivors -- they had been lucky not to have been in concentration camps and to have survived. Vegh had been luckier than many in that her mother had survived the war, and her slightly luckier situation may have made it easier for her to tell these stories, as my good recovery made it easier for me. When Vegh returned to Paris after the war and felt confused, her mother sent her to a summer camp for displaced Jewish children, and she saw herself in a new context:

'I was ashamed, I who was lucky enough to still have my mother, I didn't have the right to cry; what was happening to me? And yet I felt desperate' (Vegh, 1979: 26-27).

Vegh, like Behar, had mixed emotions of being lucky but unlucky and not understanding how to live with this paradox. She described her mixed reaction to a teacher's comment that Jewish children had come through the war so well:

'I remember feeling a certain pleasure at first: we were strong, and, in the two minutes that followed, a strange mixture of sadness, of anger and even revolt. 'It is too easy,' I thought, 'it is a way of denying all the traumas we have been through or at least of minimising them'" (Vegh, 1979: 29).

Vegh concludes her book in the middle, uncomfortable with finalities, observing that:

'Nothing was said, nothing was explained: an impression of chaos, of confusion at the heart of these families' (Vegh, 1979: 98).

"Nothing is said"

The stories I heard about polio in my research interviews were stories of paradoxes and mixed emotions -- of feeling normal but disabled, and of being strong but vulnerable. There were stories of confusion about what had happened, as polio was 'past' and must not be spoken about. Sarah, who was two when she had polio, began her interview with a story about her father:

Sarah: 'So, what I thought this morning, was that I remember something my husband said, a couple of years ago... that before he married me, my father took him aside and gave him a *pep* talk ... on how he was going to have to look after me because I got tired very easily. But at that time I was 23, I didn't think I got tired very easily. I just thought there was something wrong with *me*, and no one *talked*, no one told me that it was anything to do with the polio ... and I was stupid enough not to realise it had anything to do with the polio.'

Sarah continued to say that, "there's a strange sort of dichotomy between people thinking that you're different but not actually saying anything to *you*. That nothing is said... you're not going to be

sure about what they think at all." As with many children who had polio, this resulted in a strong determination to do as much as anyone else or more, but also "a huge fear of failure". At the end of the interview, Sarah told a story of her great-niece spilling a glass of wine at a wedding, a story of "being clumsy" that has passed down through several generations. However, it is not the story of Sarah, which cannot be told, but the story of her younger sister who was called clumsy and is remembered as complaining on the family's long country walks. Sarah ended the interview grappling to find her sister and herself in her memories of the family walks:

Sarah: ...she wanted to be in the (youngest brother's) pram. Now it could well be, I don't know, perhaps I was allowed turns, you know, I have no idea. Whether I was or not, whether she did, I don't know.

Jennie had similar shadowy memories and used the story of the disabled boy to try to clarify their meaning. Both stories resonate with my own, of untold stories leading to confusion through the generations and my own confusion about my weakness, especially as it increased over the years. Through combining these stories with my own memories in which silent members of my family fade from the picture, I could begin to grasp the pain of parents who try to protect a frightened child, or the pain of children

who are silenced to protect their parents. Because these stories cause discomfort, and are therefore difficult to tell, hear and research, this may be where social science needs to be conducted 'more subjectively so it will be more objective', through autoethnography and narrative (Behar, 1996: 29).

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Embodied ethnographic practice

Geoffrey Walford

Introduction

My current research project has a very simple purpose: to try to find out more about how experienced ethnographers actually conduct their ethnographic fieldwork and construct their ethnographic accounts. The main reason for doing the study was that I wished to gain insights from a range of ethnographers so that their views and experiences could influence a methodological text that I intend to write and provide some real examples of practice. It might be argued that to call it a 'research project' overstates its significance, for it is very small scale. The total number of ethnographers interviewed is just seven in all – but they have been selected because they have all conducted significant ethnographic studies and they have a variety of dif-

ferent disciplinary backgrounds. I wanted to include some who have an anthropological training as well as those with sociological or educational histories. As my own work has all been within education, and this is to be the focus of the methodological text, they are all 'ethnographers of education' broadly defined.

The ethnographers

The ethnographers were chosen because I value their work highly and because they represent some degree of spread of academic backgrounds, research styles and educational interests. They are also mainly people who I already knew and had a previous academic relationship with. They are, in alphabetical order: Paul Connolly, Sara Delamont, Elisabeth Hsu, Bob Jeffrey,

Bradley Levinson, Jan Nespor, and Lois Weis.

Paul Connolly has mainly worked with early years children and has focussed on issues of race and gender (Connolly, 2004). He has a chair at Queen's University, Belfast, and has recently written about quantitative research and analysis in addition to his continuing qualitative work. Sara Delamont has conducted many ethnographic classroom and school studies and has written about fieldwork methods. She has been at Cardiff University for many years where she has been Dean of Faculty and is a Reader. In my interview with her I focussed on her current research which is about teaching in a capoeira 'classroom' - a form of dance and martial art originating in Brazil which is

played, danced and fought to the music of the stringed *berimbau* (Stephens and Delamont, 2006). Elisabeth Hsu is a medical anthropologist at the Institute of Social and Cultural Anthropology at the University of Oxford. The major study (Hsu, 1999) that I interviewed her about examines the processes by which knowledge of Chinese medicine are transmitted in three different Chinese contexts. Bob Jeffrey has been working at the Open University since 1992 on various projects concerned with creative teaching and learning in primary schools, and the lives of primary school teachers (e.g. Jeffrey and Woods, 1998). In my interview with him I focussed mainly on his work on the effects of inspection on primary schools and on creative teaching. Bradley Levinson is an educational anthropologist at Indiana University, Bloomington. His major work (Levinson, 2001) has been of a provincial Mexican junior high school. Jan Nesper is also an educationist at Virginia Polytechnic Institute and State University. In my interview with him I focussed on *Tangled Up in School* (Nesper, 1997) which was the result of a two-year study of an urban elementary school, while with Lois Weis the interview mainly concerned with her series of ethnographic studies on black and disadvantaged youth in the USA (e.g. Weis, 1995). Her most recent major publication (Weis, 2004) is an interview-based follow-up of the young people involved in her 1995 ethnographic study of the white working class.

Interviewing ethnographers?

In many ways the nature of this project is decidedly odd. We know that interviewers and interviewees co-construct the interview and the replies to questions are produced for that particular occasion and circumstance. Interviewees will select their words with care (as in other formal occasions) and will moderate what they have to say to the particular circumstances. If we put to one side the epistemological question of whether or not there is any ultimate 'reality' to be communicated, the interviewee may have incomplete knowledge and faulty memory. They will always have subjective perceptions that will be related to their own past experiences and current conditions. At best, interviewees will only give what they are prepared to reveal about their subjective perceptions of events and opinions. These perceptions and opinions will change over time, and according to circumstance. They may be at some considerable distance from any 'reality' as others might see it.

How then is it possible to justify using interviews to investigate how these ethnographers conduct ethnography? Surely the only reasonable method is to observe, interview and collect our own artefacts alongside the 'subject' ethnographer as he or she goes about the task? Perhaps this would be ideal but also, perhaps, an over-investment of time and energy. While I have certainly not observed the processes by which these seven people go about doing ethnography, the fact that I have read the published ethnographies and read some of the other more methodological work of these same authors, means that there is a strong element of mixed-methods in the work. I am not simply using the results of interviews where I have no other sources of data - there is actually a wealth of written data that provides a different form of evidence about the methods used. Whilst all of these data have to be treated with care, I would argue that validity is likely to be high.

In the following section I present some preliminary considerations drawn from the data. It is far from being a full analysis, and just provides a 'taster' of some of the ideas being generated at this stage. In the presentation of the extracts from interviews below, my concern has been with clarity of communication. I have edited for meaning. In particular, I have edited out some hesitations and combined several responses - often without giving an indication that this has been done.

The body in ethnography

I paid a doctoral research student to transcribe the tapes. Apart from the increase in accuracy of transcription that his knowledge of the areas under discussion gave, it also meant that he began to theorise about the content of the interviews. He pointed out to me that 'the body' was a recurring theme in many of the interviews.

He was correct. Time and time again, these ethnographers talked about their own, often now ageing, bodies and how this influenced what they were able to do in ethnographic work and thus the data they were able to generate. Some of this was just about the physical exhaustion of doing fieldwork, or the particular pains of sitting on small primary-size chairs (Connolly), or frequent sickness caused by poor food in Mexico (Levinson), but it also had deeper implications for the research process. Some examples are given below.

The importance of the body was most obvious where the ethnography involved something that is 'physical' such as Sara Delamont's research on capoeira. The participants in the sessions she watched are all young and very fit. It would not be possible for her to become a full participant in the setting even if she wished to do so. The ageing of the physical body is such that the possible roles are limited to observation and being 'helpful' on the side.

For example, in discussing her process of observations she explained:

Geoffrey: you're standing rather than sitting?

Sara: well umh a lot of the time, yes, for three reasons. One is there often is nowhere to sit [G. right] except perhaps the floor and once I'm down on the floor I'm then not very mobile to be leaping up and the pattern of the classes is typically that the students will be in lines and they'll warm up and they'll do stretches and things like that and then they'll be taught a couple of capoeira moves by mimicry in the lines, and then they're told to go into pairs and practice them, and after that whenever the teacher wants to do any more coaching she will circle them and demonstrate with a student in the middle. Now if I'm not very mobile I can't get up and into the circle to hear what's being said and what's being taught. So I might sit, if they're in lines, I might sit where I can watch the lines, but once they're into the phase of playing in the pairs I need to be where I can move quickly. And obviously if I was a lot fitter I probably could sit on the floor and leap up but I mean, you know, um...

The personal limitations of the body even limit what it is possible to do as a helpful observer. It would have been advantageous to be able to play one of the instruments used.

Sara: And the more advanced students will be playing the instruments [G. do you sing?] I sing and I clap, [G. right ok] I haven't learnt any of the instruments. If I were more musical I would have done, because it's always useful to have people, but I'm actually very unmusical so I've resisted [G. right]. They're always trying to get me to play instruments and I keep giggling and running away. But I sing and I clap, particularly if there aren't very many people there or lots of beginners who don't know the words because

it's very important to make a lot of noise. I can't sing for toffee. I sing dreadfully flat, but I can sing loudly so I do.

In Elisabeth Hsu's study of Chinese medicine, the physical body entered as central to the subject matter, and also to her own research strategy. Her aim was to understand the transmission of embodied knowledge from practitioners to students and she felt the need to engage in what she calls 'participant experience' which meant that she wished to be involved in learning the skills and knowledge that she wished to study to such a degree that she gained the competence to perform those skills herself. She followed classes in acupuncture, the physical skills that she developed as an acupuncturist were limited. She found that she was too timid, being afraid in her needling any patients. She also found she was too clumsy and her patients complained that her hand was too heavy and avoided her where they could. Rather than the desired effects, it simply hurt. Her apprehension persisted, particularly when she was encouraged to needle in the vicinity of the eye. However, Hsu (2006) also writes about a time when her treatment of a Western woman's eczema was successful. This gave her feelings of delight and empowerment and had a crucial impact on her identity as a practitioner.

Hsu also found that her own body was an important tool in memorizing the *loci* necessary:

I pressed onto my skin, muscle and bone, and rubbed back and forth through the thickness of my clothes until I sensed, in my particular case a certain kind of sourness, *suán*, and then I loudly pronounced the name of each of the *loci*. I had learned doing so from my teachers and fellow students, and eventually, I could recite the *loci* in their sequencing along a channel (Hsu, 2006).

Whilst Hsu's experience is at one extreme, sometimes the nature of our particular bodies gives us better access to research sites and people than others might gain. Much of Lois Weis's work, for example, has crossed ethnic and class boundaries, and she seems to have had excellent access and rapport with a great diversity of people. On this occasion I asked a direct question:

Geoffrey: How much of that [your ability to work across class and ethnicity] is physical? I mean that you are a very at-

tractive woman, you are quite small, you're non-threatening. Is that right?

Lois: I think, I don't know it that helps me to work, traverse the class structure or it allows me access, I would say the latter. I mean...people want me around, now perhaps less so, I'm older, but people want me around, they're not threatened because they don't think, they often don't think that a woman will uncover anything in particular. I think that's true, even when I *tell* people what I'm doing, it goes out of their head depending on where *they're* located in the class structure, the teachers at the working class school, never acknowledged that I was a faculty member, they didn't. I kept telling them, and kept bringing it up again that I was writing a book, and it wasn't my first book. So I think yes I'm not threatening, people want me around, they're happy to show me things and teach me things, I think all that's true. I'm small.

In contrast, for Bradley Levinson, physical attractiveness at age 27 gave him a particular problem when some of the female school students began to see him as a desirable future marriage partner.

Bradley: I found out that in fact it's not at all uncommon in this region of Mexico for a 27 year old man to be courting a 15 year old girl, whereas at first it struck me as being outlandish and I realized when I started talking to parents, asking them 'when did you get married?', 'oh well, she was 16 and I was 28'. This wasn't all that crazy, you know, some of these young girls were pretty darn serious in their affections for me.

At a more mundane level, our physical bodies might be perceived as threatening by young children. Bob Jeffrey, for example felt that group interviews rather than individual interviews were more productive with primary school children.

Bradley: I found the social situation [of individual interviews] of a 58 year old, fat balding feller, sitting with a seven year old girl with a tape recorder uncomfortable and not actually very productive.

Conclusion

Of course, little of this is new. It is well known that gender and physical attractiveness can ease or hinder entry and rapport. Many other researchers have written about such issues and also about the benefits that can be gained by recognising the embodied nature of learning and of conducting ethnographic re-

search. Still, the intensity of some of the interview data concerning the body surprised me. We need to research this in more depth.

Acknowledgements

I am very grateful indeed to the seven ethnographers who willingly gave time not only to be interviewed, but also to read and correct a transcript of the interview. Thanks are also due to Nick Hopwood who skilfully transcribed the interviews.

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The Secret Life of a Research Project

Tilly Mortimore

Intentions, outcomes and the gap in between

Reading the final version of any dissertation is likely to provide little evidence of the blind alleyways, struggles and decisions that shaped it. Mine would indicate that all went as was planned from the outset and that the final version was always intended to be as it is. This, despite my apparent ability ultimately to present the design as seamless to the examiners, would be misleading. This is therefore the story of an evolving research process. It aims to cast some light on the ways in which the project and the researcher can be transformed by the lived experience and to describe the difficulties inherent in maintaining the balance between reflexive flexibility and opportunism. It charts the 5 year course of a recently completed part-time PhD project which started out as one thing and gradually became something different and greatly enriched, both in terms of the breadth of experiences included and the ways in which data was collected and analysed. It describes the range of factors, from research training, the criticisms of others, practical disasters and personal circumstances that can change what is intended and seeks to provide backing to the contention of Delamont and her colleagues that 'successful conclusions (...) are only achievable through the mutual adjustment of ideas, instruments and activities' (2000: 55). It does not aim to provide a rigorous analysis of methodological approaches or epistemologies but to share the experience of one new researcher with others in the hope of helping them in their efforts to make sense of the processes they are experiencing and of providing further confirmation of some of the experiences charted by Hallowell and her colleagues (2005).

The study was an ESRC-funded part-time studentship and had won its award with a focused empirical project. I had been a teacher working with teenaged students with severe dyslexia for many years and had become fascinated by the approaches to learning they adopted to compensate for their literacy difficulties. I had also become aware of 'common-sense' and anecdotal suggestions that learners with dyslexia might exhibit a learning style that differed from those without dyslexia and had begun to explore in my master's dissertation, the

range of cognitive and learning style literature. It was therefore important that the design should be quantitative and provide empirical evidence to prove or disprove this hypothesis.

A quantitative starting point

I identified myself as a positivist. Despite having been able to avoid studying science at school, I still saw myself as a scientist and scientific knowledge as universal, quantifiable, empirical and predictive. I aimed to isolate and observe phenomena, then use statistics to make reliable inferences. At the start of my studentship, therefore, my project was a simple quantitative comparison of cognitive style in a sample of 60 dyslexic and 58 non-dyslexic male university students gathered from 17 universities. I was aspiring towards high standards in my research design (see Gorard, 2001). The sample would be random; all variables within the two groups would be matched so that one alone, dyslexic status, could be manipulated through a finely focused experimental task. Students would be categorised as dyslexic or not by psychometric assessments, and cognitive style was operationalised in terms of quantitative scores on the Cognitive Styles Analysis (Riding, 1991), a computer-presented series of tasks that measured cognitive style. I also planned to investigate the relationship between dyslexia, cognitive style and students' ability to utilise information delivered in a lecture task by requiring all participants to watch a carefully designed video lecture and respond to questions which would measure their levels of recall. In terms of the great qualitative/quantitative divide, which seems, in my experience, to remain contentious, I was on the side of the experimenters. My quantitative design was ready. I knew exactly the direction of the research journey and thought I knew the statistical tools I would take with me.

The impact of research training

I had not bargained for the impact upon the project of the ESRC research training diploma, which I was obliged to complete in the first year of my PhD. This year also happened to be the only full time research year I was allowed and it was therefore essential that I should design the study, prepare instruments and begin to gather data before having to return to my daytime job. I was there-

fore not keen to 'waste' time. The training was, however, to be a catalyst for change in the design. Silverman (1997) introduced criticism of the logical-positivist thrust of my quantitative approach and of attempts to apply scientific methods to social science research with people. I encountered post-modernist contentions that science is simply one among many "constructs"; post-structuralist challenges to the hegemony of the language of science and profound doubt as to the applicability of scientific tasks and methods involving reliability and validity to social science research with human beings. The intention to rely upon a carefully crafted artificial task and statistical tools began to feel less satisfying. Hammersley (1990) has described ethnographic research as being developmental and exploratory – a journey away from the original focus towards new conclusions and accordingly I found myself cast adrift from the project's focus upon 'induction' or upon findings based upon observations. The initial paradigm upon which the design was constructed was being undermined.

Rudestam and Newton suggest: 'Good research is a constant balancing act between control and meaningfulness' and warn against the risk of sacrificing one for the other (1992: 29). I began to wonder whether my carefully controlled experiment, which had already necessitated eliminating women participants to reduce the number of factors in my statistical analyses, was going to provide me with the insights I wanted. These critiques of my positivist stance and the discovery of new paradigms had set the project off on a voyage into a new, somewhat threatening manifestation of academic reality; new because the whole thrust of my previous academic apprenticeship had aimed to create an empirical, detached observer, threatening because, it placed my subjective reality in a more central position and described it in personal terms that would have previously, within the empirical paradigm, been considered inappropriate. This made me feel vulnerable, as if I were dropping the researcher's protective cloak of invisibility.

These doubts were bound fundamentally to affect the plans for the study. However there were pressures from a number of quarters to maintain the original ap-

proach. The design of the project submitted to and funded by the ESRC had been a quantitative experiment. The aim had been to investigate anecdotal reports and provide empirical evidence to confirm or deny their validity. I still held strong reservations as to the validity, reliability and generalisability of information taken from smaller scale qualitative studies, reservations that were echoed by my supervisor, a psychologist and an expert in statistical analysis. I had wanted to structure a study that would produce reliable and valid findings which could be applied to larger groups of people. Despite my growing awareness that quantitative findings could indeed mislead, qualitative methodology did not seem to offer these opportunities of generalisation. I was not at this point in a position to replace the quantitative paradigm with another, or even to begin to adapt the paradigm. The study remained resolutely quantitative.

The impact of practical obstacles

Three factors now combined to confirm my doubts and begin to shift the balance. One was practical. My supervisor had been urging me to consider interviewing participants. He kept making the point that if I had managed to get them to attend, I should elicit as many data from them as possible, that this would enable enrichment through triangulation without compromising the validity of the study. The experiment also required a 'filler' activity which would provide a memory decay gap in the experiment between watching the video-taped lecture and answering questions on the content. I therefore designed a questionnaire about the participants' learning difficulties and experience of learning support which I regarded not as a way of entering their world but as an empirical instrument. As a side issue, it would provide me with information about the participants, who were still at this stage regarded as 'subjects' within an experiment.

The second factor was the harsh reality of finding participants. However, once I started to collect my data, I understood exactly what my supervisor had been trying tactfully to say to me. Gate keepers and access formed one type of hurdle but, once they were overcome, I thought students would be arriving in batches, twenty in an afternoon. In reality, I would drive 100 miles to a university to find that I was lucky if two had remembered to turn up. I would then have two choices, to give up in despair or to be flexible and adapt to the situation. There

was no real choice. The questionnaire was ready; I had been trained in interview techniques; I had more time to spend with each individual so the questionnaire became a structured interview and I began to communicate with people and hear their stories. The project began to gather some rich and fascinating data. I became aware that participants are as precious as diamonds. The transformation of the project from quantitative to qualitative was under way. But, was this reflexive flexibility or the opportunism my quantitative training had warned me against?

Intellectual justification for practical measures

An answer to this could be suggested by the third factor that came into play at this point. This arose again from Hammersley's (1990) and Silverman's (1997) descriptions of ethnographic research methodology encountered through my research training. This began to enhance my understanding of different ways in which the qualitative data now being gathered might meet my criteria of validity and to suggest that it is possible to check for a common consensus of relevance and validity by examining the plausibility and credibility of evidence and judgements through cautious analyses of cumulative observations. These researchers highlighted the potential of qualitative data for pilot studies or debriefing, and for investigating further ideas that emerge from pilot questionnaire surveys. In addition Spradley (1979) endorsed generalising from small scale in-depth study through the use of a "grounded theory" (Glaser & Strauss, 1967) which extracts empirical ethnographic data by techniques of domain analysis, taxonomic analysis, componential and theme analysis. The potential for the use of these rigorous, if not exactly quantitative, ways of analysing the qualitative data I had collected from my interviews increased my confidence in the possibility of the project going beyond purely anecdotal evidence and of the scope for generalising from this type of information.

The project had always been emancipatory in its aim of helping higher education institution support services to provide the best ways of removing barriers to access for dyslexic learners at university. Following the realisation that qualitative and quantitative methods could share the same rigour, it seemed increasingly as though, despite warnings about the "uneasy relationship between quantitative and qualitative data" (Delamont,

1984: 22), human insight and depth could be injected into the project by enriching the quantitative approach with a more ethnographic phase, with a more qualitative word-based individualised investigation. This seemed justifiable if not advisable. It was not until much later, however, that I would be reassured by Tashakkori and Teddlie (1998). They describe the possibilities offered by the combining of quantitative and qualitative analysis and it seemed clear that this combination might enable the experiences of students, with and without dyslexia, to be examined in ways that merge statistical analysis of the distribution of responses to the cognitive style measure and the lecture task with narratives describing students' experiences in the world of higher education. This could provide another way of looking at the stories underpinning the quantitative data and illustrate the experiences of dyslexic students and their understanding of these experiences; to 'grasp the native's point of view' (Malinowski, 1922: 25). I had been forced by circumstances to adapt the study but now found methodological justification for the changes in the nature of the design.

The final design

By this stage in the process, the completed dissertation, which had originally been launched in an empirical form, had evolved and been enriched by the researcher's experience of the process. It still had, at its core, an experiment with findings that have been analysed statistically and found to be significant and reliable within the experimental paradigm. There were, however, further challenges to this at times during the writing, caused not only by my extreme difficulties in mastering statistical analyses but by the publication of a harsh critique (Coffield et al., 2004) of the constructs underpinning cognitive style in general and of the empirical reliability and validity of my chosen instrument in particular (*The Cognitive Styles Analysis*, Riding, 1991). This coincided with the completion and first dissemination of data derived from this instrument and challenged the foundations of my quantitative methodology.

For these reasons, there were moments when the qualitative aspect of the study might have taken precedence and, indeed, papers relating to this dimension are both published and in preparation. The participants' voices are rich and the findings are striking but what is significant to me as a somewhat lapsed positivist is the fact that this aspect of the study

is underpinned and confirmed by the results of the experiment. To provide just two examples: the dyslexic students state that they have difficulty with absorbing information. Their judgements about themselves are confirmed by their significantly lower scores on the lecture when compared with their non-dyslexic peers. The dyslexic learners claim that they take a long time to process information – they perform significantly more slowly than the controls in the experiment. People do not always do what they say, however honest they may try to be. The study needed to measure the participants' performance on recall tasks to discover whether students with dyslexia really did have more difficulty with recall than students without dyslexia. This could only be evidenced by comparing scores on an identical task with a control group of participants without dyslexia. The dyslexic participants state, in both questionnaires and interview, that they experience difficulty. Statistical analysis indicated that there were significant differences both in the perceptions of difficulty between students with and without dyslexia and between the performance scores from the recall task. The quantitative corroborates the qualitative. What they say is echoed by what they do and, without the experiment stage of the study, it would have been hard to demonstrate this. This seems to provide strong evidence for the research hypothesis.

I am convinced that the doctoral dissertation that was finally submitted is both richer and more satisfying than the one originally embarked upon. It draws from both traditions and each enriches the other. As the project mutated, so did the researcher. I am still attracted by quantitative methodologies. My research journey, which has not always been straight forward or easy, has however, reshaped me as a researcher, forced me to reframe my theoretical constructs, to reject ear-

lier limiting assumptions and to embrace with enthusiasm new qualitative methodologies. In the process, I am certain that I have become more open-minded, more thoughtful and better able to support the range of masters students that I am now supervising across the variety of studies that they undertake. As the project began to change and to relinquish some of the constraints set upon it by my original mindset, so I feel that I have evolved into a more creative and flexible researcher.

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After experience in comprehensive schools Tilly Mortimore set up English departments at two specialist schools for dyslexic learners. She has lectured internationally, led training programmes and consultancy support in dyslexia in a range of educational settings. Her 2003 book "Dyslexia and Learning Style" provides an outline of research and suggests practice to support learning preferences. After completing her part time PHD at Cardiff University in 2006 and two years' lecturing at Southampton University, she is moving to a lectureship in Inclusion at Bath Spa University. Her research interests include emotional aspects of dyslexia, inclusion and learner identity.

Erratum

In the article 'Transparency in the derivation and construction of fictionalised narrative' by Tony Rea (Issue 3, Summer 2006), a section of the first narrative was not printed. We apologise for this mistake and are printing the correct narrative here:

At once Kirsty breaks into a clapping dancing chanting routine learnt, she tells Rod, at Girl Guides.

"So, what's been the best part of Hallwell Lodge, Kirsty?"

"Staying away from my mum and dad," she replied.

"Did you like that?"

"No. But I know I can do it. I'm not scared of the sea now, either. I just smiled when I swallowed a bit of the water."

"Anything else?", Rod asked.

She thought for a while. She glanced at Johnny and Josh.

"I put my hand up more, I've not been such a scaredy cat!"

News and Forthcoming Events

Qualiti workshops

<http://www.cardiff.ac.uk/socsi/qualiti/events.html>

Qualitative Research and Ethical Approval

Queens University, Belfast

14 March

This one-day workshop will consider the practical implications of obtaining ethical approval for undertaking qualitative research in the social sciences.

Multi-modal Qualitative Research

Plymouth University

30-31 May

Two-day workshop is primarily based on the combined use of textual, visual and audio data. It will give participants practical insights in to the advantages and disadvantages of different modes of qualitative data.

Real Life methods node workshops

Comparative Cases

University of Manchester

1 March

Details can be found at:

<http://www.reallifemethods.ac.uk/events/workshops/comparative-cases/>

Qualitative Longitudinal Research Training Workshop.

London

16 March

For further details:

<http://www.reallifemethods.ac.uk/training/workshops/>

Other UK events

AHRC Workshop Series

Centre for Qualitative Research, Bournemouth University

Social Science in Search of its Muse; Exploratory Workshops in Arts-related Production and Dissemination of Social Science Data

7 March - Workshop 3 **Visual Ways of Knowing**

8 May—Workshop 4 **Rhythm and Blues: Turning to the body**

Further information at:

<http://www.bournemouth.ac.uk/ihcs/rescrahrhc06.html>

Training Researchers to Engage with Public Policy (TREPP)

London

18 April — **Using documentary evidence**

18 April — **Elite Interviewing**

12 June — **Appraising research critically**

TREPP is a major new programme of training supported by the ESRC Research Development Initiative (RDI). More information on each course is available at:

<http://evidencenetwork.org/training03.html>

Autoethnography masterclass

Bournemouth

Autoethnography pioneer, Carolyn Ellis, will be conducting a two-day masterclass in Bournemouth on the 10th & 11th April, 2007. Advance information and booking procedures by emailing:

cqr@bournemouth.ac.uk

ESRC Festival of Social Science

9-18 March

Showcasing the work of the social sciences to a

broad audience. For further information visit the ESRC website: <http://www.esrcsocietytoday.ac.uk/ESRCInfoCentre/about/CI/events/FSS/>

British Sociological Association Annual Conference 2007

University of East London

12-14 April

It will include a session on methodological innovation

For further information visit the conference website at:

<http://www.britisoc.co.uk/events/Conference.htm>

CAQDAS 2007 Conference: Advances in Qualitative Computing

Royal Holloway, University of London

18-20 April

This conference is an opportunity to once again bring together methodologists, users, developers and trainers of software designed to facilitate qualitative research, to discuss and share methods, applications and experiences.

For more information visit the conference website:

<http://caqdas.soc.surrey.ac.uk/conference/conference07.htm>

International conferences and workshops

4th International Qualitative Research Convention, Doing Qualitative Research: Processes, Issues and Challenges

Malaysia, PJ Hilton

3-5, September, 2007 (Deadline for proposal submissions 30th March, 2007). For further details:

<http://qrc2007-gram.um.edu.my> ;

<http://www.qram.org/> or email to:

qrc2007@gmail.com

The 3rd International Congress of Qualitative Inquiry

University of Illinois, Urbana-Champaign

May 2-5, 2007

Participants will explore the politics of evidence and truth and what these terms mean for qualitative inquiry in the new century.

Details: <http://www.qi2007.org>

International Visual Sociology Association

Annual meeting "Public Views of the Private; Private Views of the Public", August 10-12, 2007 in at New York University (NYU) in the true heart of New York City -- Greenwich Village. More details will be available shortly at:

<http://www.visualsociology.org>

Practising Narrative Analyses

Tampere, Finland

25-26 June 2007

International postgraduate course. The course is intended for postgraduate students in social sciences and cultural studies with a methodological orientation toward narrative studies. It offers participants a unique opportunity to work on their own research projects with the guidance of internationally esteemed experts on narrative theory and research. The course is organized in connection with the 3rd Tampere Conference on Narrative, "Knowing, Living, Telling" (27-30 June 2007 in the Tampere Hall). The course may substitute as an alternative access to the conference in the sense that no separate conference paper is required. For details see:

<http://www.uta.fi/conference/3narrative/precourse.htm>

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